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in their place'' is the element that I miss in his logic and in that of others of his school. My incidental criticism, to which I still adhere, is that while it ''maintains its relation to the metaphysics from which it has been obtained, it has lost its connection with science.'' I desire to add, however, that the (admissions and) explanations to be found in Dr. Bosanquet's discussion, if they are more fully developed in subsequent work, will go some way towards remedying the deficiency.

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CAUSE AND GROUND. A REJOINDER

MR. SHELTON has kindly shown me his paper, and suggested that I should make an addendum. I fear it would be difficult for us to come to an agreement; for Mr. Shelton, as I judge from his foot-note 2, approaches philosophy rather from the outside, and demands that its treatment of its material shall be adapted to his questions. And I think it very likely that it will not be able to satisfy him, even if, as I believe, its treatment and material are fuller than he has trained himself to recognize. Offering more than is easily grasped causes misapprehension, as well as offering too little.

The question is, he says, whether such a treatment of causation as mine serves a useful purpose. Well, what is a useful purpose? Mine, in this case, is to satisfy a great human interest by helping to clear up the nature of knowledge. His, I fear, is to subserve the progress of natural sciences. I see nothing more useful in the latter of these than in the former. I do not think Mr. Shelton would say ''useful = conducive to 'practical' interests.'' If he did, we should have to drop our discussion till we could talk out pragmatism. I did think he had leanings that way because of his demand for practical science from a branch of philosophy, and that was why I held pragmatism relevant.

But he may take me on the true ground of philosophy, and say that I *don't* help to clear up the nature of knowledge. Now I think that he really has not quite seen how entirely relevant my argument was. For he supposes that my references to tautology, and to the descriptive view of science, and to what he thinks ''a totally different metaphysical doctrine,'' the doctrine of ground, are irrelevant. But here, I submit, he has not quite got inside his subject. For the point lies in the unavoidable transformation of the conception of ''cause and effect'' according to the phase of common sense or of science with which we may be dealing. And all the points I referred to lie

well within the catena of meanings which must be thus constructed—that is to say, the actual significations of cause and effect, or the corresponding ideas (*e. g.*, in geometry, where cause and effect can not be used at all), as they are employed every day in the practise of the sciences. There is no question of the metaphysical unity of the universe or any heterogeneous conception. The phases described are the actual practise of science, concealed from the common-sense public by bad popular tradition.

Let us test the matter by the importance to be assigned to repetition. I said that the important thing for science was reference to a ground, *i. e.*, systematic determination. Mr. Shelton says it is repetition. This, in my view, is just the popular fallacy that induction lies in generalization from one or a few cases to many. It is wholly opposed to the practise of science and the theory of the best logicians. Take the fact that water boils at 212° Fahr. at sea level. After one strict experiment, the repetition of this fact is absolutely without scientific interest. The interest lies in the further development of the facts and theory of barometric pressure or the volatilization of fluids. In such a development cause *ipso facto* passes into ground. We no longer speak of things and events, but of laws and systems of conditions.

All I have done is to interpret the inductive theory of cause and the real practise of science. This, I submit, is more convenient, as well as nearer truth, than to work with a conception like cause which changes in your hands at every step in scientific progress. This is clearly what Clifford meant, and it is the whole tendency of the science of biology, as it approaches, on the one hand, the organic, and, on the other, the mathematical ideal. The category of cause can not be used in either of these types of knowledge. It belongs to the level of common sense and elementary observation.

It is very disagreeable to me to seem to defend the merits of my own "Logic." But really I am speaking here of the whole tendency of modern logical theory. Of course my own book is full of defects. Still, it has the outline of the doctrine which is the *a, b, c* of the modern theory of knowledge—that the sciences create their own methods for their own purposes, but yet these methods are mere working hypotheses, good in so far as they work, but differing greatly in their claim to anything like truth. There is no "admission" nor a step in advance in this doctrine. It is the very *raison d'être* of logical theory.[1]

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[1] Any one interested in the detail of the theory of causation would do well to refer to Mr. Joseph's "Introduction to Logic," Clarendon Press.